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Feminist Philosophy of Science: Standpoint Matters

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Feminist standpoint theory has a contentious history. It is an explicitly political as well as social epistemology, characterized by the thesis that those who are marginalized or oppressed under conditions of systemic inequity may, in fact, be better knowers, in a number of respects, than those who are socially or economically privileged. Their epistemic advantage arises from the kinds of experience they are likely to have, situated as they are, and the resources available to them for understanding this experience. *Feminist* standpoint theorists argue that gender is one dimension of social differentiation that makes such an epistemic difference.¹

Standard critiques of feminist standpoint theory attribute to it two manifestly untenable theses: that epistemically consequential standpoints must be conceptualized in essentialist terms, and that those who occupy them have automatic and comprehensive epistemic privilege. A world structured by hierarchical, oppressive social divisions thus becomes a world of unbridgeable epistemic solitudes. I agree that neither thesis is tenable and I argue that neither is a necessary presupposition of standpoint theory. The anxious philosophical nightmare of corrosive relativism² does not afflict standpoint theorists any more than it does other varieties of social epistemology and socially naturalized contextualism, and need not be epistemically disabling in any case. My aim here is to offer a systematic reformulation of standpoint theory, and address two questions: What epistemic insights does standpoint theory offer? And what is the scope of its application?

I. The challenge: Why standpoint theory?

To start, consider the kinds of epistemic problem to which feminist standpoint theory is a response. When contemporary feminist standpoint theory took shape in the 1970s and early 1980s, the catalyst was not strictly philosophical. That is, standpoint theorists were not chiefly concerned with internally generated questions about hypothetical possibilities in a space of epistemic positions defined by centuries-old philosophical tradition. Rather, they were responding to epistemic questions raised by second wave feminist research and activism. The challenge they took up was to make sense of the impact that feminist interventions had had on well established research programs in a wide range of fields, destabilizing conventional assumptions about sex/gender, bringing to light limitations and errors in well established research programs that had gone unremarked and often, in the process, opening up productive new lines of inquiry. This seemed to fly in the face of conventional epistemic wisdom; how was it that these pivotal insights, critical and constructive, should arise from research informed by an explicitly political angle of vision? It was in response to the transformative effects of these feminist interventions that standpoint theorists explored a cluster of contextualist claims about the ways in which the social, specifically gendered, identity of knowers can affect not only their recognition as epistemic agents but also the epistemic resources and capacities on which they draw as knowers.³ Here, then, are a few examples of contexts in which, three and four decades ago, feminist theorists encountered and explored the epistemic effects of gendered socialization, experience, relations of production and reproduction

First, as activists in the 1960s and 1970s, feminists struggled to identify and to name patterns of disenfranchisement and inequity that had gone unrecognized or, indeed, were vehemently denied. One example, recently discussed by Miranda Fricker, was the difficult process of characterizing what is now understood to be “sexual harassment” (2007, 149-152). She focuses on the ways in which “hermeneutical lacuna” – a gap in the collective hermeneutic resource “where the name of a distinctive social experience should be” (2007, 150) – can create an “asymmetrical disadvantage for the harrassee” (2007, 151). The famous case of Carmita Wood⁴ that Fricker considers at length illustrates how the dearth of conceptual resources for recognizing this experience was a self-reinforcing effect of gendered hierarchies of power, a form of structural discrimination that was hard to detect precisely because it had not previously been named as such.⁵ It also illustrates how, in the process of addressing this hermeneutical disadvantage, those affected by it drew on experiential evidence and articulated conceptual resources that put them in a position to delineate an empirical phenomenon,⁶ a “middle range fact” as Mary Morgan puts it,⁷ the dynamics and effects of which have proven to be as stubbornly persistent as they are consequential.

By the mid-1980s, grass-roots activist research on workplace environment issues had extended the insights articulated in cases like Wood’s to a number of other types of subtle, largely unremarked gender dynamics by which women found themselves persistently undervalued and marginalized even as overt policies of exclusion were struck down. From the mid-1970s through the early 1980s Mary Rowe (1974), and Roberta Hall and Bernice Sandler (1982, 1984, 1986), introduced the metaphors of “micro-inequities” and “chilly climates,” bringing into focus the effects implicit bias that operates through small-scale, often unintended and unrecognized gender differences in uptake and response, recognition and evaluation.⁸ In the late 1980s Arlie Hochschild coined the term “the second shift” (also “double work day”) to capture the impact of gender norms that maintain traditional divisions of labor in the household and even ramp up the domestic demands on women as they moved into the wage labor market in increasing numbers (1989 [2003]).⁹ The metaphors of “glass ceilings,” “leaky pipelines”, and a “gender tax” on women’s wages were coined in the same period, and in the early 1990s Margaret Rossiter proposed the term “Mathilda effect,” inverting Merton’s “Matthew Effect” (Rossiter 1993), to capture persistent patterns of cumulative disadvantage that arise when “micro-inequities” operate unchecked in supposedly meritocratic systems.¹⁰

The contours of these phenomena emerged with great difficulty, the hard-won outcome of collective processes of critical reflection and investigation that took root in many different contexts. Time and again you see references to a “shock of recognition” (Aisenberg and Harrington, 1988, ix; Brownmiller 1980, 280), when the process of comparison threw into relief strikingly similarities in experiences that women had assumed were idiosyncratic, drawing attention to recurrent patterns of difference in recognition and authority, mechanisms of deflection and marginalization (including sexual objectification and harassment). The struggle to articulate experiential insights that do not fit conventional expectations is palpable – a matter of coming to terms with the compounding effects of hermeneutical lacunae, testimonial injustice, and willful ignorance. But in many cases these metaphorically invoked inequities and the mechanisms that produce them have proven to be robust social phenomena; they have become a staple of empirical investigation, the target of equity policies and of activist interventions. Research programs initiated in the 1970s have since documented systemic gender inequities using national databases and field or sector-scale analyses. Their contributions include, for example, economic models of persistent gender differences in income that control for a wide range of potentially confounding factors (Ginther 2004); finegrained analyses of career paths that delineate exit patterns and choke points specific to particular types of training pipeline and career ladder (Xie and Shauman 2003); models of age- and rank-graded patterns of cumulative disadvantage that arise within field and institution-specific career paths, of the kind that identified in the famous report of the MIT Committee on Women Faculty in the School of Science as “post-civil rights era discrimination” (MIT

1999). In addition, since the 1960s research programs have taken shape in sociolinguistics and in cognitive and social psychology that have produced a rich array of insights into the nature and workings of internalized cognitive schemas that shape a variety of default assumptions and automatic responses, to do with race, ethnicity, linguistic and national affiliations, religion, age, and sexuality, as well as gender.¹¹ These have brought into view a repertoire of now well-documented mechanisms that operate below the threshold of conscious awareness – various forms of evaluation bias, attribution bias, stereotype mobilization, group dynamics, and norms of recognition, uptake, response – the effects of which ramify into the large scale, cumulative gender differences in outcome invoked by the metaphors of a gender tax, glass ceilings, leaky pipelines, and the “Mathilda effect” that feminist activists posited in the 1970s and 1980s.

The transformative effects of distinctively feminist critiques and insights extended well beyond research that explicitly addressed questions about gender inequity. A remarkably diverse range of feminist research programs took shape in one after another social, historical science and, increasingly, in the biological and medical sciences, putting significant pressure on everything from key empirical claims to orienting assumptions about specific subject domains, from localized methodological norms, to field-defining ideals of scientific practice.¹² Consider a few examples that illustrate what these research programs involved and influenced the formation of feminist standpoint theory.

Within sociology, Dorothy Smith advocated a program of ethnomethodological research designed to document and theorize how the “everyday world” looks to those who primarily operate off-stage, in gender-normative roles that put them disproportionately in the position of maintaining social relationships and collective physical well being (Smith 1974). One illustration of this approach was a series of studies she undertook with Alison Griffith of the ways in which school day routines and the work organization of women’s lives amplify rigidly gendered parenting responsibilities: the tutoring, lunch making, and other voluntary contributions of labor and resources expected of mothers (1987, 181-187). She was interested in “the concrete actualities” as well as the ways women conceptualized “mothering as work” (Howard 1988, 21). As an influential standpoint theorist, Smith advocated a set of methodological strategies for recovering dimensions of the social world that are typically “eclipsed” (1974, 7; 1987, 17-36): whatever the social context under study, she urged researchers to start from the perspectives and experience of those who are marginal, ask what they understand, work with their cognitive schemas and categories. This approach was intended to counter the imposition of categories that reflect the situated experience and assumptions of sociological outsiders; it puts the researcher in a position to learn how social relations, institutions, conventions actually operate, and to recognize their effects as “ruling practices” (1974, 8) that are often not visible to those who operate “center stage,” who benefit from the status quo and who largely define the agenda of social sciences. This was, in effect, exactly what Chilly Climate activist researchers were doing at a grass-roots level, taking their dissonant experience as a point of departure for understanding the ways in which meritocratic systems actually operate.¹³

Another prominent sociologist, Patricia Hill Collins, emphasized the distinctively raced as well as gendered insights of social science insider-outsiders. She brought a critical perspective to bear on mainstream research that, despite trenchant critiques, continued to reproduce the kinds of racial and class bias associated with the Moynihan report and its pathologizing characterization of Black family structure and “culture of poverty” (Gans 2011). She describes the “mismatch” between her own working-class Black experience and the “taken-for-granted assumptions of sociology” about “the family,” “human capital,” and the causes and effects of poverty (1991, 47-54), showing how the questions asked, the data gathered, the analytic and interpretive resources on which sociologists typically rely to make sense of their data embody their own dominantly white, middle class experience, as understood through the lens of masculine roles. Her constructive response takes the form of a systematic reassessment of core sociological concepts that reflects the social and economic realities

navigated by black women; she articulated a resolutely intersectional genre of standpoint theory.

Perhaps most influential in philosophical contexts is Carol Gilligan's critique of Lawrence Kohlberg's psychological models of moral maturation, a key source of inspiration for the articulation of an ethics of care.¹⁴ Her point of departure was concern that Kohlberg's system was based exclusively on interviews with boys, obscuring a distinctively contextual and relational mode of reasoning about moral problems that, while not exclusively associated with girls, did surface more prominently in their responses (1987). But more broadly, she questioned Kohlberg's assumption that there must be a single, universal trajectory of moral development, a conviction that led him to treat recalcitrant counter-evidence – the non-conforming responses of girls – as anomalies, evidence that girls were maturing more slowly than their male counterparts. Her posit of a "different voice" has itself been sharply criticized on grounds that, for example, she maintained a broadly Piagetian conception of maturation; she shared with Kohlberg the assumption that distinct modes of moral reasoning reflect discrete developmental stages and did not explore the possibility that some of these might, instead, constitute a repertoire of situationally-attuned responses that are elicited by changes in context and inflected by race and class difference (Fraser and Nicholson 1990, 133; Heyes 1999, 154). Nonetheless, the strategy of critique she deployed in "creating a space for girls to be heard" (Heyes 1997, 149) threw into sharp relief systematic gender bias in the selection of subjects, the framing of analytic categories, and the interpretation of empirical findings that had gone unrecognized until she brought the resources of a feminist perspective to bear. Although she had significantly reframed her own alternative account within five years of the publication of *In a Different Voice* (1982, 1987), her original critique demonstrated the need for nuanced attention to gendered dimensions of difference among moral subjects that had not been taken into account except as evidence of deficiency.

Critical challenges to gender-conventional assumptions arose not only in the context of research on contemporary subjects in familiar contexts, but also in the study of historically and culturally distant others. The historian Jane Kelly-Gadol drew attention to how different the "Renaissance" looks if you attend to the fortunes of women. It was anything but a period of cultural rebirth for women, indeed, she argues, there was "no 'renaissance' for women, at least not during the Renaissance; there was, on the contrary, a marked restriction of the scope and powers of women" (1976, 811). Moreover, she argues that this was a direct consequence "of the very developments for which this age is noted," and reflects "a fairly regular pattern of relative loss for women precisely in those periods of so-called progressive change" (1976, 810). As Kelly-Gadol develops this critique, it is not just a brief for reexamining the specifics of the period we conventionally refer to as the Renaissance, but for a thorough-going reassessment of the periodization scheme in terms of which European history has been written (1977).

In a similar spirit, by the late 1960s and early 1970s feminist anthropologists had demonstrated how different things look ethnographically if you attend to the activities, language, relationships, and perspectives of women; they were intent on reclaiming women's words and cultural worlds, focusing on the private, domestic dimensions of well-studied cultural contexts that had been eclipsed (to use Smith's language) by a preoccupation with the public roles and activities of men that were taken to stand for the cultural whole. Much was accomplished but almost immediately the substantive insights generated by this program of remedial research began put pressure on the assumptions about gender difference and separate spheres in terms of which it had been conceived. A series of auto-critiques appeared in the late 1970s and early 1980s in which feminist anthropologists scrutinized the familiar sex/gender binaries they had projected onto culturally distant, typically non-industrialized cultures. Michelle Rosaldo argued, in an especially trenchant analysis that appeared in 1980, that the gendered conception of a "domestic" sphere, and the sharp segregation of private from public, was itself a product of the formation of a distinctively Euro-American middle class in the late 19th century. The ethnocentrism of assuming that these categories are salient in other cultural contexts could not but

undermine the feminist ambition of grasping the diversity of ways in which sex/gender differences are marked (or not) across the range of cultures that had been the subject of ethnographic study. It would be necessary to significantly broaden the scope of feminist critique, shifting attention from the challenges of “adding women” to more subtle and far-reaching questions about framework assumptions (Rosaldo 1980, 390).¹⁵ From this it follows that the potential relevance of feminist analysis extends well beyond the investigation of explicitly gendered subjects. Reflecting on the implications of women’s history, which followed a similar trajectory, Peter Novick notes that “in principle, feminist perspectives in history [proved to be] as relevant to such male activities as war and diplomacy as they were to realms in which women dominated,” even though in practice, these implications often remained sharply circumscribed (1988, 496).

Finally, one domain where the convergence of several lines of feminist analysis has had a profound impact is in challenges to theories of human evolution which take it for granted that “the demands of the hunt shaped the characteristics that make us human” (Dahlberg 1981, 1). The impetus for rethinking conventional “man the hunter” models came, in part, from a reassessment of research on what had been known as hunting, or hunter-gatherer, societies (Lee and Devore 1968). Ethnographers who had turned their attention to the roles and activities of women “gatherers” learned that, in sub-tropical, desert, and temperate regions, their foraging activities provide small game and plant resources that account for as much as 70% of the dietary intake of the group as a whole. Indeed, they found that when women captured small game it was described as having been “gathered” or collected, whereas it was recorded as “hunting” when attributed to men (Zihlman 1997, 100). They learned, as well, that women are by no means sedentary and dependent on resources provided by their male counterparts; there are often gender differences in mobility patterns, but women range widely and, given the distinctive breadth of their foraging activities, they have especially comprehensive knowledge of regional ecology. The result is that women play a key role determining group movement when these decisions turn on considerations of resource availability, exercising forms of power and leadership typically presumed to be the exclusive domain of men (e.g., Sally Slocum 1975; see overviews by Dahlberg 1981, and Fedigan and Fedigan 1989).

In the first instance, these reassessments of “hunting” societies inspired the corrective of “woman the gatherer” models of human evolution according to which it was the reproductive advantages conferred by the social and cognitive skills required for success in women’s traditional foraging activities that drove human evolution. But within a decade internal critique made it clear that this female-centered antithesis was just as problematic as the sexist and androcentric models of human evolution it was intended to displace; a more fundamental conceptual realignment would be required if accounts of human origins were to adequately reflect what had been learned about the flexible, active roles of women and children in foraging societies (Zihlman 1997, 96-99, 109). The debate is ongoing, but it seems clear that the sex/gender roles observed among contemporary foragers, much less those characteristic of the post-industrial societies in which most researchers have been socialized, cannot plausibly be projected onto deep prehistory. Parallel lines of critique arose from reassessments of what had been assumed about primate social dynamics. Field research that systematically documented the activities of female primates demonstrated that they are by no means passive coquettes, dominated by and dependent upon aggressive male strategists. But neither does the inversion of these assumptions do justice to flexibility and diversity of behavior that was increasingly being reported by close observers of contemporary primates. As Susan Sperling puts it in a memorable assessment of this debate, “langurs with lipstick are no improvement over baboons with briefcases” (1991, 27). Reflecting on the implications of such anthropomorphism for evolutionary theorizing, she argues that “the new female primate... dressed for success and liv[ing] in a troop that resembles the modern corporation” (1991, 4) is no more adequate a framework for understanding contemporary primates, or ancestral primate and hominid populations, than the stereotypes of an earlier era. In short, the impact of bringing a critical perspective to bear on the gendered, and also class and race-inflected assumptions that had framed

research in these various fields was a growing appreciation that our hominid and proto-hominid ancestors most likely lived in social groups and depended on subsistence strategies that were unlike any that are familiar from primatological or ethnohistorical research in contemporary contexts.¹⁶

The pattern that emerges repeatedly in the trajectories by which feminist research programs took shape is that, although they often began as modest interventions, almost invariably the process of filling gaps and correcting biases drew attention to deeper, more pervasive problems. In some cases, remedial research raised questions that could only be addressed by considering new lines of evidence and by expanding the range of conceptual resources brought to bear in the analysis and interpretation of this evidence. Within history, records that had been considered ephemera – diaries, pamphlet literature, various forms of material culture – were crucial in tracing the historical fortunes of women, among other disenfranchised subjects of “history from below.” Attention to women’s experience and gendered dimensions of everyday life were a key source of insight into dimensions of social experience that had been missed by ethnographers, or were systematically occluded by conventional census and survey research. By contrast, it was by reframing the analysis of existing national employment and census data – bringing new questions to bear and probing for patterns that hadn’t previously been investigated – that feminist economists and quantitative sociologists were able to trace the cumulative effects of small scale differences in opportunity and reward, fleshing out the contours of pipeline effects, a gender tax, and cumulative disadvantage. And it was a process of critically scrutinizing the background knowledge (itself empirical) in terms of which primary paleontological data had been interpreted as evidence that put feminists in a position to reframe evolutionary theorizing. As remedial research took shape, drawing attention to pervasive patterns of error and distortion in a wide range of fields, it became clear that the challenge was not just to add missing pieces to an existing puzzle but to reframe the puzzle as a whole, with ramifying implications. I have focused here on the human, social sciences, but there are striking parallels with feminist interventions in the medical and life sciences;¹⁷ I return to questions of scope at the end.

The upshot is, then, that feminist interventions catalyzed transformative criticism¹⁸ in a remarkably diverse range of fields over quite a short period, from the late 1960s through the 1980s. This had the effect, not just of redirecting empirical inquiry, modulating methodological norms, and calling into question entrenched framework assumptions but, as Novick has emphasized with respect to history, it also destabilized conventional ideals of objectivity conceived in terms of a vernacular positivism: the conviction that epistemic success in empirical research is characterized by, and should take as its primary goal, convergence on a single, comprehensive, true understanding of the world, and that a necessary condition for such success is the insulation of empirical inquiry from the influence of social, contextual values and interests.¹⁹

In contexts dominated by these epistemic ideals, the kinds of critique and, crucially, the constructive contributions made by feminist research programs pose a significant epistemic challenge, one that was often recognized, commented on, wrestled with by practitioners themselves. How could systematic error and distortion have arisen and persisted so long in research programs that had been conducted with integrity, that were apparently impartial with respect to social and political values, and that could claim significant empirical and explanatory success? Often the target of feminist critique was not manifestly inadequate science of the kind that, for example, Elisabeth Lloyd has documented in connection with selectionist accounts of human female orgasm (2005) but, rather, the ubiquitous partiality of good science, science as usual, even our best science, as Harding put it in the mid-1980s (1986, 102). By extension, why was it that, with respect to gender bias, it was women and most often feminists who noticed these incongruities, subjected them to critical scrutiny, and initiated the empirical research programs that identified and counteracted of sexist and androcentric bias? In contexts where objectivity was presumed to be irrevocably compromised whenever contextual values or political commitments intrude, this last raised a particularly thorny set of questions.

II. The response: Initial formulations

It was in response to these challenges that feminist standpoint theorists asked: What would a theory of knowledge look like if you took seriously the possibility that entrenched, systemic inequalities in our material and social conditions of life can be epistemically enabling? What are the implications of taking gender and other social identity categories as a basis for questioning the ways in which epistemic agency had been idealized? These are, I contend, the epistemic challenges of our time. I suggest that the feminist standpoint theories formulated in the late 1970s and early 1980s are best understood as an exploration of resources for answering these questions in philosophical and scientific contexts where any association of empirical success with identity politics was deeply incongruous.

Two types of explanatory resource figure prominently in the initial formulations of feminist standpoint theory that appeared in the 1970s and early 1980s. To account for how distinctive epistemic resources could arise from gendered identities and social relations, Marxist feminists posited gender-specific modes and relations of production, and those influenced by object relations theory appealed to the psychoanalytic processes of infantile gender socialization. Nancy Hartsock makes use of both in her influential essay, "The Feminist Standpoint: Developing the Ground for a Specifically Feminist Historical Materialism" (1983).

As Hartsock's title suggests, her point of departure was the insight from Marx that, only by adopting the perspective of those dispossessed by exploitative, class-structured relations of production is it possible to grasp "what is really involved in the purchase and sale of labor power" (1983, 287), how surplus value is generated, how class-based inequalities in power and resources are perpetuated, and how these processes are rationalized and mystified. On Marx's account, the source and ground of a distinctive proletarian standpoint – the critical angle of vision that confers these epistemic advantages – is the kind of practical, embodied activity associated with this class position. Those who must sell their labor, who lack control of the means of production and must navigate a class-structured world from a position of relative powerlessness, have direct experience of social realities that those in positions of structural advantage can ignore, and that are systematically obscured by a dominant ideology that serves to legitimate exploitative relations (1983, 285-288). Hartsock reframes these arguments in terms that make "the 'gender differentiation of labor'... a central category of analysis" (1983, 307, citing Young 1980, 185), and that capture the epistemic implications of hierarchically structured gender relations of production and, crucially relations of reproduction. She argues that "women's work...differs systematically from men's" (1983, 289), and that this sexual division of labor is fundamental, not reducible to or derived from more basic conditions of class difference. Women make a "dual contribution to subsistence in capitalism" (1983, 291); not only do they labor for wages, but they are delegated primary responsibility for household production. Women are thus immersed in the concrete labor required to sustain a "complex relational network" (1983, 293); they produce, socialize, and maintain the well-being of other human beings, a labor that primarily generates use-values rather than commodities that have exchange value.²⁰

This classic feminist reformulation of historical materialism provides a framework for understanding how systematic gender differences arise in our social relations and in practical, material life activities and, crucially, how they are maintained as structural conditions. But to explain how individuals internalize these conditions such that they become the ground for a distinctively gendered psychological and cognitive orientation, Hartsock appealed to object relations theory. This, she argued, supplies a mechanism, in the form of early childhood socialization, that, under conditions of a sexual division of labor in child rearing, accounts for the formation of gendered conceptions of the self that are so deeply entrenched they are experienced (and rationalized) as natural. Following Chodorow and Flax, Hartsock

argued that, when the primary caregivers are women, male children are put in the position of articulating their masculine identity “in opposition to another who threatens one’s very being” (1983, 296), the female caregiver upon whom he is dependent but from whom he must differentiate himself; indeed, they must learn masculinity through identification “with an abstract, cultural stereotype” (1983, 295). By contrast, femininity “is concrete for girls,” given the continuity of identification with a female caregiver; a characteristically feminine self emerges that is said to be experienced in relational terms, less threatened by interdependence and afflicted by fewer anxieties about “boundary challenges” (1983, 294). Hartsock saw in this early drama of individuation the psychological foundation that underpins institutionalized gender differences in relations of production and reproduction (1983, 295-296); gender differences in identity and psychological orientation in turn reinforce divergent patterns of “material life activity” and associated experience (1983, 299). This, she argues, “has important epistemological and ontological consequences for both the understanding and the construction of social relations” (1983, 299); it is the basis for a distinctive standpoint that has the resources to “go beneath the surface of appearances to reveal the real but concealed social relations” of a gender as well as a class structured society (1983, 304). Hartsock here articulates a claim of epistemic privilege that I refer to, in what follows, as an “inversion thesis”: the thesis that certain kinds of epistemic advantage accrue to those who are otherwise (socially, materially) disadvantaged, in this case by systemic gender as well as class difference. If plausible, this sketch of the structural, socio-economic conditions and psychological mechanisms that entrench gender differences in labor and social relations explains how those socialized as women could acquire a body of experience and, in some cases, develop a critical, reflective (feminist) angle of vision that put them in a position to recognize and to critically scrutinize androcentric assumptions that had been taken for granted in a great many mainstream research programs.

Evelyn Fox Keller had made much bolder claims for standpoint theory five years earlier in a famous essay, “Gender and Science” (1978, reprinted in 1985). Relying exclusively on object relations theory, she substantially extended the Chodorow and Flax line of argument, invoking gender differences in “intellectual posture” that arise from infantile socialization to explain an “historically pervasive association between masculine and objective, [and] more specifically between masculine and scientific” (1978, 415 and 409). On her account, masculine identities are forged through a “quasi-universal” process of psychological maturation that involves, centrally, a struggle on the part of male children to dissociate from their female caregivers. This, in turn, creates a predisposition in those socialized as men to enact a “radical dichotomy between subject from object” which she finds characteristic of canonically objective scientific thought (1978, 424).²¹ In *A Feeling for the Organism* (1983), Keller offers an account of what she took to be a contrasting style of intellectual engagement manifest in the work of Barbara McClintock, the geneticist whose work on gene transposition in maize had long been inscrutable to her colleagues but who had, in the end, won a Nobel prize. She characterized McClintock’s special “powers of discernment” as a form of “intuition resting on sympathetic understanding” (Keller 1983, 200-201) which arose from a capacity to immerse oneself in the object of study, to “forget yourself” such that “the objects become part of you” (1983, 118). So described, McClintock’s practice embodies an intellectual posture “associated with stereotypically feminine gender traits” (Richards and Schuster 1989, 700). It is striking in retrospect that, at the end of her 1978 essay, Keller acknowledges that variation in research styles among male scientists, and changes in patterns of parenting have the potential to undermine this argument “linking [the] scientific and objective with masculine” (1978, 430-431). These were exactly the grounds on which her claims about McClintock and her association of science with a distinctively masculine epistemic orientation were pilloried by feminists and nonfeminists alike.

The critical reaction was immediate. Steven J. Gould raised the first of the two potential objections acknowledged by Keller in his review of *A Feeling for the Organism* (1984). He could identify any number of successful male scientists whose practice embodies the sense of identification, of caring

immersion in the world of the phenomena, that Keller had attributed to the intellectual posture of those socialized as women. Two years later Harding, initially an advocate of standpoint theory, published an influential formulation of the second objection drawing on colonial counter-examples to challenge the empirical adequacy of the psychoanalytic thesis that a distinctively masculine as opposed to feminine epistemic orientation is rooted in infantile gender socialization (1986, 179-185). She noted pervasive similarities between the race and gender metaphors that figure in attributions of distinctive ways of knowing to members of African cultures and to women. In both cases, these “others” are characterized as immersed in the phenomenal world; they are relational subjects who see themselves as continuous with the objects of their knowledge, dominated by affect rather than rationality and incapable of the impartiality necessary for properly objective inquiry. This “curious coincidence,” Harding argues, decisively undermines the psychoanalytic thesis: “we certainly cannot explain the African vs. European dichotomy by appeal to the infant’s experience of the division of labor by gender,” inasmuch as child care is no more predominantly the domain of men in African than in European societies (1986, 185). It is more plausibly the effect of a common, underlying cause; the raced and the gendered “other” is defined, in both cases, in opposition to the characteristics of a normatively white, male, European epistemic agent, the unmarked term in a hierarchy of social categories. To understand “differences in cognitive styles and world views,” Harding went on to argue, rather than posit fundamental and universal differences in psychological orientation, it makes better sense to attend to the kinds of social relations and concrete activities in which epistemic agents are typically engaged within hierarchical systems structured by gender and race as well as class difference (1986, 189).

These objections to psychoanalytic formulations of standpoint theory were quickly generalized to all forms of standpoint theory.²² Harding herself concluded at this juncture (1986) that standpoint theory represents an unstable, internally contradictory position mediating between naïve feminist empiricism and feminist postmodernism; if the constructivist insights associated with standpoint theory are taken seriously, they compel a shift to the latter, more radical position. In the event, few were willing to follow Harding’s lead in embracing a resolutely ironic, postmodern stance. She herself later endorsed a form of standpoint theory that incorporates the methodological recommendations articulated by Smith, and emphasizes the epistemic effects of social relations of production, much as Hartsock had, and as she had recommended in response to Keller (Harding 1991, 1993).

Within a decade, dissatisfaction with early formulations of standpoint theory was intensified by a virulent reaction against all forms of identity-based politics and scholarship. Two lines of critique emerged in the “identity politics wars” of the 1990s that have profoundly shaped the fortunes of standpoint theory. The primary objection, anticipated by Gould’s reaction to Keller, was that standpoint theory presupposes a reified, essentialist conception of social identity: the posit of a distinctively gendered “intellectual posture” makes sense only given an implausibly reductive conception of women’s interests and identities. In making the case that “one is not born, but rather becomes a woman,” as Beauvoir famously put it (1952, 249),²³ feminists themselves had effectively demonstrated the contingency and the diversity of women’s roles and identities; if the category “woman” has no anchor in essential attributes shared by all of its members, what basis could there be for appeals to a women’s or feminist “standpoint,” or for that matter, any social identity-defined standpoint? The problem with this argument is the assumption that, unless social identities can be grounded in social kind categories that are exclusive, sharply bounded, stable, and internally homogenous, they dissolve into limitless individual diversity. In a trenchant analysis of the philosophical and political underpinnings of the identity politics debates, Linda Alcoff (2006) challenges the assumption that there must be one “generic and general sense” in which social identity can be salient – typically one that turns on an untenable reification of within-group sameness (2006, 86) – otherwise social identities have no conceptual or empirical substance, at least none that can bear epistemic weight (2006, 42-45).²⁴ This reflects a profound failure of imagination, Alcoff argues: a failure to recognize and to theorize a range of possibilities that lie between these implausible extremes. “Visible identities,” collectives based on ascribed and affiliative

identities, can be powerfully consequential in all kinds of ways – materially, socially, politically – despite being historically contingent, porous, and internally heterogeneous (2006, 85-87). Lines of social differentiation, articulated by social identities, do not have to be “quasi-universal” to make a systematic difference to the life opportunities, patterns of concrete activity, social relations and identities of individuals. One consequence of the inability, or refusal, to take seriously the “powerful salience and persistence” of collective identities (Alcoff 2006, 87), has been a deep-seated skepticism about their capacity to be in any way socially and epistemically enabling.

The second standard objection to standpoint theory targets the “inversion thesis,” drawing out the epistemic implications of an essentialist conception of social identities. The presumption here seems to be that if one’s location in a field of socially differentiated identities makes a philosophically interesting epistemic difference, it must be a difference in what counts as evidence and in norms of reasoning that constitute the basis for adjudicating knowledge claims.²⁵ In this case, the members of different social groups must see the world in ways that are incommensurable with one another. The threat of disabling relativism is never far from the surface in these discussions, raising the specter of collectives that have no basis for settling their epistemic differences except by insisting, by fiat of political or social force, that one world view (and its aligned epistemic norms) takes precedence over the others. On this picture, if epistemic advantage is to be attributed to any group, it must take the form of a claim of automatic epistemic privilege: those who are members of oppressed or subdominant groups are credited with knowing more – knowing how things really are – strictly by virtue of their social, political location. Feminist standpoint theory was thus condemned as a form of epistemic ‘political correctness’ gone badly wrong; it recapitulates hackneyed stereotypes of “women’s ways of knowing,” inverting patterns of testimonial injustice in a bid to claim epistemic credibility. In the process, standpoint theorists risked undermining their own political and epistemic goals, undercutting any claim that they might have for wider recognition of the insights generated by their critiques of inequity and research programs.

It is hard to find a standpoint theorist who embraced an essentialist conception of social identity or a formulation of the inversion thesis that attributes automatic privilege to the dispossessed, much less any who endorsed the reductive relativism associated with these theses. Such views are, however, routinely attributed to standpoint theorists, especially in philosophical contexts where crude formulations of standpoint theory figure prominently as a stalking horse for critics who categorically reject feminist philosophy of science or feminist epistemology as a contradiction in terms and a cynical brief for politicizing knowledge.²⁶

There is certainly much to criticize and rethink in early formulations of standpoint theory as, indeed, feminists theorists had themselves been quick to recognize. The appeal to object relations theory for a mechanism that accounts for a “quasi-universal” gendered orientation to objects of inquiry is certainly too crude to be plausible. But note that even this most maligned aspect of feminist standpoint theory took the form of a hypothesis about the contingent effects of caregiving practices characteristic of a particular family structure and gendered division of labor. Keller herself acknowledged that this was an empirical claim (see note #20), and subsequent research has made it clear that, although processes of gender identity formation are by no means inconsequential, they are much more malleable and context specific than object relations theorists originally recognized. Thanks to the growing body of research on cognitive schemas and non-cognitive processing mentioned earlier, we now have much richer resources for theorizing the diverse ways in which social norms are internalized and influence our behavior along any number of dimensions; crucially, this includes an increasingly nuanced understanding of the specific (and variable) content of the schemas we internalize and the contextual factors that modulate their effects. In a similar spirit, Hartsock clearly understood her historical materialist posit of a fundamental division of labor along gender lines to be a contingent feature of a particular social formation. It was an ambitious thesis that has since been significantly complicated and refined by intersectional analysis, but rather than providing grounds for categorically rejecting

standpoint theory, these critiques reinforce the insight that context-specific analysis is required to understand exactly how our “cognitive styles” and epistemic resources are affected by the material and social conditions of life that function, to varying degrees, as structural features of the social contexts in which we operate. It is, in short, an open and empirical question whether, in any given context, systems of social differentiation obtain that are robust enough to canalize our lives and shape our identities in ways that make a difference to our capacities as epistemic agents.

What most concerns me, however, is not the adequacy of now-conventional critiques of standpoint theory; rather it is that charges of essentialism and of automatic privilege miss the mark on a more fundamental level. They ignore the epistemic challenges posed by the emerging programs of feminist activism and research that motivated initial formations of feminist standpoint theory, and they provide no constructive response of their own to these challenges. Feminist research programs continue to thrive, evolving through the processes of internal auto-critique that I described at the outset and, as they do, the puzzle deepens of how to account for the contributions made by transformative criticism grounded in identity-specific forms of experience, angles of vision, and “standpoints.”

III. Standpoint theory reframed

In taking up these challenges now, I propose a reformulation of standpoint theory that neither assumes nor entails the untenable theses attributed to past formulations, and that draws some key lessons from the protracted debate I have described.

First, I propose that standpoint theory is best construed as a purpose-specific epistemic stance; it is not a full-service epistemology, an alternative to the conservative (empiricist) and radical (postmodern) options that Harding identified when she mapped the epistemic landscape of feminist theory in the mid-1980s. As Kristen Intemann has argued, standpoint theory is most plausibly understood as compatible with a sophisticated feminist empiricism (2010); it requires an account of cognitive-social norms of epistemic adequacy capable of underwriting a discerning assessment of the specific kinds of epistemic advantage that may be conferred by structurally defined social locations. Nor is standpoint theory just a set of methodological maxims, as Smith has been inclined to claim (1997). Her injunction to “start inquiry from the margins” presupposes an epistemic rationale that standpoint theory aims to capture. Standpoint theory is, rather, a conceptual framework that directs attention to a set of jointly descriptive and normative questions about the impact of systematic social differentiation on our epistemic capacities, on what we know (well) as situated epistemic agents. The answers generated in response to these questions constitute a genre of radically non-ideal theory (Mills 2005); standpoint theories, so conceived, not only describe and explain the impact of identity-based epistemic difference on existing research programs, they also identify conditions that are conducive to transformative criticism going forward.

Second, I identify three social-epistemological theses that comprise the conceptual framework within which specific standpoint theories are formulated, each of which is open to a variety of formulations depending on the specific target of analysis.

- (1) A generic situated knowledge thesis. The point of departure for standpoint theorizing is a recognition that there is no “view from nowhere”; contingent histories, social context and relations, inevitably affect what epistemic agents know (including explicit knowledge as well as tacit experiential knowledge), and shape the hermeneutic resources, inferential heuristics, and other epistemic resources they bring to bear in generating and adjudicating knowledge claims.
- 2) A systemic situated knowledge thesis. What distinguishes standpoint theory from other genres of social epistemology predicated on a generic situated knowledge thesis is its focus on the epistemic effects of systemic structures of social differentiation. Standpoint theorists are concerned to

understand the impact, on what we know and how we know, of our location in hierarchical systems of power relations that structure our material conditions of life, and the social relations of production and reproduction that, in turn, shape our identities and our epistemic capacities.

3) A thesis of epistemic advantage. Finally, standpoint theorists are particularly interested in the kinds of epistemic advantage that may accrue to those who are socially marginal, exploring various reformulations of the much despised inversion thesis.

In embracing these stance-defining theses, standpoint theorists need not commit, in advance, to any particular substantive account of the structural factors that constitute the social worlds in which knowers are situated or of the psychological mechanisms by which these shape their identities and cognitive capacities. In particular, they need not assume that, to be socially and epistemically consequential, these structural conditions must be “quasi-universal.” It is, as indicated earlier, an open (empirical) question whether such structures obtain in a given context, what form they take, and how they are internalized or embodied by individuals. So conceived, standpoint theorizing proceeds on the assumption that systematic patterns of social differentiation, and the social identities based on them, cannot be presumed to be epistemically irrelevant. Feminist standpoint theorists are characteristically suspicious of any program of inquiry that “disappears gender” as a relevant dimension of epistemic inquiry.²⁷

Likewise, the distinctive standpoint claim about epistemic advantage on the margins – the inversion thesis – need not, indeed, should not be construed as an attribution of automatic or comprehensive epistemic privilege to members of subdominant social groups. Any number of feminist as well as critical race and class theorists have made the point that social, economic disadvantage is often constituted by and imposes epistemic deficits. For example, as Uma Narayan observes, the oppressed may have immediate experience of how systems of exploitation operate but are “denied access to education and hence, to the means of theory production”; they may see more clearly certain realities and effects of class, race, and gender divisions, but may not have “a detailed causal/structural analysis of how their specific form of oppression originated, how it has been maintained and all the systemic purposes it serves” (1988, 35-36). Here, again, it is an open question whether the features of social location that make an epistemic difference confer epistemic advantage, what these advantages are, and whether they are relevant to specific epistemic projects. The inversion thesis, reframed in terms of differential epistemic advantage (rather than automatic privilege), takes the form of a directive to raise these questions, and a caution against reproducing, in epistemic inquiry, conventional forms of testimonial injustice. It articulates a recognition that those who are disadvantaged by structural conditions may well have epistemic resources that the comparatively privileged lack.

As this suggests, the kinds of epistemic advantage that standpoint theorizing brings into focus vary widely.²⁸ Most prosaically, those who are socially marginal may be privy to evidence, and may develop the interpretive heuristics necessary to understand and to navigate dimensions of the social and natural world that the comparatively privileged rarely engage, or are invested in avoiding. More controversially, distinctive forms of knowledge may arise from non-mainstream social and cultural traditions that have taken shape quite independently of those that constitute dominant culture. Finally, the experience of exclusion or marginalization may itself be a source of insight. Various forms of critical dissociation and comparative meta-knowledge become possible, indeed, necessary when survival as an insider-outsider requires that you understand the norms of a dominant culture as well as those that structure your own subdominant community. The dissonance described by Collins (1991, 49-50), the sociological ruptures and “fault lines” exploited by ethnomethodologists like Smith (1987, 49), and the “double consciousness” made famous by W. E. B. Du Bois (1993 [1904], 7-15) are all examples of the kinds of experience that can put those who are socially marginal in a position to recognize what remains tacit for members of a dominant culture, in the process catalyzing counter-narratives and counter-norms that have the conceptual resources, lacking in dominant culture, to name and to make sense of this

dissident experience.

This last type of epistemic advantage throws into relief a third and final feature of standpoint theory: the recognition that a standpoint is, as Kathi Weeks puts it, “a project, not an inheritance; it is achieved, not given” (Weeks 1996, 106). The resources of situated knowledge may give rise to but do not, in themselves, constitute a standpoint. Hartsock argues that “a standpoint is not simply an interested position (interpreted as bias) but is interested in the sense of being engaged” 1983, 285).²⁹ In the sense developed here, a standpoint is characterized by a particular kind of epistemic engagement, a matter of cultivating a critical awareness, empirical and conceptual, of the social conditions under which knowledge is produced and authorized. Standpoint theory concerns, then, not just the epistemic effects of *social location* but the effects and the emancipatory potential of a critical *standpoint on* knowledge production. So conceived, the project of standpoint theory is to systematically investigate the epistemic effects (in both senses) of immersion in different kinds of social relations and concrete activities within systems of hierarchically structured social differentiation. This is engaged philosophy in the further sense that its goal is to put the insights generated by standpoint analysis to work in assessing, calibrating and improving the cognitive-social norms that govern the epistemic practices on which we depend for action-guiding knowledge of the social and natural worlds in which we live.

IV. Standpoint theory: scope and practice

To illustrate standpoint theory at work in these two senses – in analysis of the effects of situated knowledge and of a reflexive standpoint on knowledge production – and to address questions of scope, I close with two brief archaeological examples.

The first is a standpoint-informed analysis of the emergence of “gender research” in archaeology. This was a strikingly late development compared to those described at the outset. It was not until the mid-1980s that archaeologists took an interest in questions about gender and developed feminist critiques of androcentrism and sexism in the research priorities, interpretative conventions, and modes of representation typical of archaeology, and it was not until the early 1990s that the first major publications appeared, some 25 to 30 years after feminist research programs took shape in such closely aligned fields as history and cultural anthropology. Intrigued by this development, I undertook a series of interviews with the organizers and a survey of participants in the first major international conference on “The Archaeology of Gender,” convened in Calgary in 1989.³⁰

At the outset I assumed that the catalyst for gender archaeology must have been feminist analysis informed by activist engagement and exposure to the feminist scholarship that had flourished in neighboring fields. As Linda Gordon put it in a retrospective assessment of feminist history, one “hardly need mention that the feminist retellings of the past [were] stimulated by feminist political challenges to present-day structures and relationships” (1986, 21). Certainly, those who published the initial critiques, organized the first conferences, and assembled the first edited volumes were self-identified feminists and well versed in feminist scholarship. But a very different picture emerged from a content analysis of the abstracts (Kelly and Hanen 1992), and from the results of my survey.³¹ I learned that, although women were disproportionately represented on the program of the 1989 conference (80% of the presenters were women, at a time when women comprised 36% of the membership of the Society for American Archaeology), and although three-quarters of respondents reported that they had a pre-existing interest in questions about gender (this was the reason they attended the conference), few had a background in women’s studies or feminist scholarship, and a majority made it clear that they did not equate an interest in gender with any kind of feminist commitment or affiliation; more than half the women and a larger majority of the men were explicit that they did not identify as feminists. Probing further I discovered that, while the average age of the women attending this conference was similar to

that of the men (43 compared to 40 years, respectively), their distribution across age cohorts was very different; 60% of the women (twice the proportion of men) were clustered in the 26-40 year age range. This meant that a majority of the women had entered the field in the late 1970s, completed their graduate training in early 1980s, and established professional careers by the time of the conference – in a period when the representation of women in North American archaeology had more than doubled.

As members of the first cohorts in which a critical mass of women had entered archaeology, the women who were drawn to the 1989 “Archaeology of Gender” conference seemed acutely aware that their very presence in the field posed a challenge to conventional assumptions about women’s roles and capabilities, even as they disavowed feminist affinities. I hypothesized that the dissonance they experienced in their professional working lives threw into relief gender norms that not only structured the institutions of archaeology but were presupposed by the conceptual framework of the discipline. The Chacmool conference gave them an opportunity to engage these issues—to critically scrutinize gender-normative assumptions—on the intellectual terrain of scholarly inquiry, as a subject of archaeological inquiry. In short, I hypothesized that, while a majority of those involved distanced themselves from a feminist standpoint, they drew on the resources of situated knowledge; their contributions to the formation of gender archaeology reflected a largely untheorized and apolitical awareness of the contingent nature of gender roles that arose from their positioning in the field as professional insiders but gender outsiders.

The discomfort with identity-based (feminist) scholarship and activism expressed by many participants in the 1989 “Archaeology of Gender” conference has since intensified into an explicit repudiation of feminist influences. In the last decade, several prominent advocates of gender archaeology have objected that feminist “political overtones and associations” have compromised this research program, contributing to its marginalization within mainstream archaeology where they find it viewed “with suspicion” (Sørensen 2000, 5).³² They aggressively defend ideals of objectivity that equate epistemic credibility with the impartiality of a presumed “view from nowhere,” precisely the epistemic norms that underpin the objections that provoked their response: that gender research must be feminist, and therefore inherently biased. This is a deeply conflicted defensive stance, not just because the archaeology of gender showed little more engagement with feminist scholarship after a decade than was evident in 1989, but also because it effectively denies the originality of the contributions of a research program that arose, not from business as usual, but from the critical insights of women whose dissident experience put them in a position to recognize, and counteract, the unremarked androcentrism of mainstream archaeology. In their concern to defend the epistemic integrity of gender archaeology, its advocates do not consider the possibility that the conventional ideals of objectivity might, themselves, need to be reconceptualized to account for the situated epistemic agency that mobilized gender archaeology. I elsewhere propose a reframing of ideals of objectivity that give central place to a consideration of the epistemic virtues that make for good quality knowledge, indexed to problem and context of use, and to features of epistemic practice that are most likely to foster these epistemic virtues procedures – the social-cognitive norms of community practice.³³ But this is a topic for another time.

This pattern of circumscribing the impact of presumptively feminist interventions, declaring them no threat to conventional epistemic norms and ideals, is evident in a number of contexts, especially in connection with debate about the implications of equity critiques. As one trenchant critic of gender inequities in the physical sciences puts it, the institutions and professional culture of these sciences may be profoundly sexist, but “photons have no gender” (Urry 2008); inequities in the application of meritocratic norms—a reliance on working indicators of rational authority that relegate women to the margins, that impose glass ceilings and reinforce leaky pipelines—have no bearing on the conceptual, empirical integrity of the science. The assumption seems to be that, if the subject domain is not gendered, critiques that purport to identify the effects of gender bias in the content of the science can

get no traction. A second archaeological example illustrates how systemic patterns of inequity in a discipline can have substantial epistemic impact of just this kind.

This final case is an analysis of the gender politics of Paleoindian research developed by Joan Gero, one of the early, explicitly feminist catalysts for gender research in archaeology (Gero 1993).³⁴ Paleoindian cultures had long been characterized as highly specialized large game hunters, defined archaeologically in terms of the tool kits and distinctive kill sites associated with hunting Pleistocene mammoth and bison. The central problematic of Paleoindian studies was, then, to explain what happened to the Paleoindians when the Pleistocene mega-fauna went extinct; had they been replaced by broad spectrum foragers, or somehow undergone a dramatic transformation? On Gero's account, this was a resolutely male-dominated field, characterized by a focus on stereotypically male activities; she documented patterns of gender segregation within Paleoindian studies in which men focused on hunting assemblages, replicating technically sophisticated Clovis points and associated hunting and butchering practices, while women chiefly worked on expedient stone tools, doing edge wear and residue analysis. She also found that women in the field were cited less frequently than their male counterparts, even when they worked on the same material, unless they published with a male co-author. As a result, the evidence that women were producing of a diverse range of foraging activities, based on close analysis of the smaller blades and flake tools presumed to be associated with women's activities, had largely gone unnoticed. Not only was the account of Paleoindian subsistence practices incomplete, but the defining research agenda of the field, the "bison-mammoth knowledge construct" (1993, 37), was an artifact of androcentric biases implicit in the characterization of Paleoindian subsistence in terms of a continent-wide "Clovis adaptation." The whole conceptual framing of the field had to be rethought.

Although this is a case in which the subject domain is gendered, it illustrates a strategy for standpoint analysis that extends straightforwardly to non-gendered subject domains. If gender segregation is entrenched in a scientific field, as it is in many, and if patterns of professional uptake and response reflect evaluation bias (e.g., in citation, collaboration, funding), then it is to be expected that the questions women ask and the results they generate in the disciplinary niches where they typically work will get less recognition, and will have less impact on the trajectories of research in their fields than do the contributions of men, operating in comparably better supported and more influential research niches. In cases like these, systematic patterns of testimonial injustice translate into a self-reinforcing canalization of research effort that has the potential to shape everything from the specifics of research practice to dominant styles of reasoning and core framework commitments: what will count as significant questions and as compelling answers to them. There is no need to invoke an implausibly generalized "women's way of knowing," or the dynamics of ethnocentric projection specific to fields that deal with gendered subjects, to recognize that gender inequities can generate a cascade of content effects that raise serious questions about the epistemic integrity the field. This suggests that the ratification of empirical and theoretical results as objective, as knowledge claims we can trust, must be informed by a systematic assessment of how well the epistemic resources of diverse, situated epistemic agents have been incorporated into their adjudication; social-cognitive norms of community practice must bring a critical standpoint perspective to bear on the processes knowledge production if they are to be epistemically credible.

In conclusion, then, I argue that we need a robust standpoint theory to understand the epistemic effects of systematic social differentiation, including both the effects of situated knowledge and the conditions that foster transformative criticism. I submit that these lines of analysis apply as directly to philosophy as a discipline as to any empirical science.

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Notes

¹ The analysis of standpoint theory presented here originated in Wylie (2003), and various aspects of the argument have been developed in more detail in Wylie (2011a, and 2012).

² I invoke, here, Alan Richardson's discussion of the "anxious nightmare of people wholly incomprehensible to one another," a form of "philosophical hypochondria" (from Peirce), that haunts contemporary philosophy by which, as he describes it, any weakening of commitment to epistemic foundationalism is presumed to entail an inescapable slide into epistemic nihilism (2006, 9).

³ Transformative criticism originating in feminist research programs was by no means an isolated phenomenon. Class-based Marxist analysis had catalyzed an earlier generation of standpoint theories on which feminists drew (see the discussion of Hartsock that follows), and the development of race critical theory has a parallel trajectory, one that has had a profound impact on feminist theorizing.

⁴ Carmita Wood's legal battle, and the pivotal role it played framing the concept of sexual harassment and mobilizing feminist activism, is described in detail by Brownmiller (1990; as cited by Fricker, 2007, 149).

⁵ Recent critiques and extensions of Fricker's analysis bring into sharp focus the extent to which gaps in the hermeneutical resources of a dominant culture are not inadvertent, and should not be assumed to imply a lack of resource on the part of the subdominant communities whose experience is misrecognized (Mason 2011; Dotson 2011).

⁶ For an assessment of the debate about experiential evidence, see Wylie (1987).

⁷ I draw a parallel here with Morgan's recent analysis of the process by which the concept of a "glass ceiling" went from a metaphor to what she describes as a well defined "middle range fact": a persistent pattern of blockages in the career advancement of professional women into the most senior positions (2010).

⁸ For further discussion of chilly climate research see Wylie (2011a), Wylie, Jakobsen and Fosado (2007), and the Chilly Collective (1995).

⁹ Hoschild opens a new edition of *The Second Shift* (2003, originally published in 1989) with the observation that "the number of women in paid work has risen steadily since before the turn of the century, but since 1950 the rise has been staggering": from 30% in 1950 to over two thirds by 2002 (2003, 2).

¹⁰ Merton's reference was to Matthew (13:12): 'For whomsoever hath, to him shall be given, and he shall have more abundance; but whomsoever hath not, from him shall be taken away even that he hath.' Where Merton had focused on the effects of cumulative advantage in academic contexts, Rossiter drew

attention to a gendered pattern of cumulative disadvantage. She named this principle the 'Mathilda Effect' in honor of the 19th century suffragette, Mathilda Gage, who developed an early critique of the ways in which women's contributions to collective knowledge had been ignored or appropriated (1993).

¹¹ I have in mind here the investigation, in a number of different contexts, of the mechanisms of automatic cognitive processing responsible for the apparent irrationality of common problem-solving strategies, much of which developed quite independently of the feminist research programs I have described. Valian provides a comprehensive review of such research the aim of which was to bring its results to bear on the equity issues addressed by feminist activist/researchers (1999).

¹² The summary that follows draws on Wylie (1997a), and is outlined in the sections contributed by Wylie to Wylie, Potter and Bauchspies (2012 [2009]).

¹³ The recommendatio that feminist social scientists should take seriously the concepts and assumptions that are salient within a context – they should “start from the margins,” or “think from women’s lives” as Harding puts it (1991) – should not be understood to imply an uncritical endorsement of the understanding of insiders. For an assessment of debate about this methodological principle, see Wylie (1992).

¹⁴ For an especially valuable analysis of this debate see Behnabib (1992), and for an overview of the various genres of “care ethics” that were inspired by Gilligan within a decade of the appearance of *In a Different Voice*, see Held (1993, chapter 3).

¹⁵ See, for example, Harding’s discussion of the limitations of remedial research programs focused on “women worthies,” “women victims,” and “women’s contributions” in the range of fields cited here (1986, 30-31).

¹⁶ Haraway’s account, in *Primate Visions* (1989), of the formation of primatology and its enganglements with military and capitalist interests is perhaps the most influential and expansive critique along these lines, key elements of which appeared in the early 1980s. See, as well, Fedigan (1986), for discussion of the gender dynamics of the field, and contributions to *Primate Encounters* (Strum and Fedigan 2000) for a retrospective assessment of these debates.

¹⁷ For a recent overview of “gendered innovations” in the physical and life sciences and engineering, see contributors to Schiebinger (2008).

¹⁸ I use the term “transformative criticism” in the sense explicated by Longino (1990, 70-73).

¹⁹ This presupposes, of course, ideals of science as value-free or value-neutral, impartial, and context-transcendent, predicated on standard distinctions between fact and value, fact and theory, and between the context of discovery and that of verification (or justification).

²⁰ Hartsock observes that, “unlike men, women’s lives are institutionally defined by their production of use-values in the home” (1983, 291).

²¹ Keller is careful to note that she takes the differences between masculine and feminine “minds” to be a function, not of “biological differences between male and female brains,” but of developmental processes (1985, 79-80). She asks: “How we are to account for our adherence [to the belief that] the associations between scientific and masculine are simply true?” (1978, 415). This passage, among others, suggests that what she means to explain are robust and ubiquitous *perceptions* of gender difference that certainly influence behavior but may also obscure diversity in behavior and cognitive style that does not fit the stereotypes. In this spirit, Jane Roland Martin argues that Keller should be read as offering an analysis of cultural constructs of masculinity and the ways in which symbolically masculine traits are associated with science (1988, 135-137). Reflecting on Gould’s critique (see below), she observes that “we do not have enough evidence to know if the different style does or does not characterize the scientific practice of one sex more than the other” (1988, 135).

²² While Harding’s critique of object relations theory provides compelling grounds for rejecting the psychoanalytic formulations of standpoint theory, it leaves untouched the historical materialist dimensions of Hartsock’s account (Wylie 1987).

²³ As Beauvoir puts this point in the introduction to *The Second Sex*: “The biological and social sciences no longer admit the existence of unchangeably fixed entities that determine given characteristics, such as those ascribed to woman, the Jew, or the Negro” (Beauvoir 1952, xiv).

²⁴ The further worry articulated in this debate is that if social identities are, in fact, consequential – if they do shape the lives and identities of individuals – this can only be because they are an opportunistic political construct, coercively imposed on group members in ways that undermine the integrity of individual agency. When Alcoff addresses the question of why social identity categories were

conceptualized in these starkly polarized terms, she offers the diagnosis that this reflects an underlying anxiety about identity categories of any kind: that they are “foisted on the self from the outside, by the Other” (2006, 81), grounded in a static, self-warranting set of interests external to the individual and therefore inevitably compromising, never enabling their autonomy and flourishing as individuals.

²⁵ For an analysis of this line of argument as represented by Boghossian (2006), see Wylie (2011b).

²⁶ Consider, for example, Haack’s characterization of feminist philosophy of science and epistemology, originally published in 1993 and reprinted several times, without amendment, most recently in Pinnick, Koertge and Almeda (2003). For a systematic critique of such accounts see Anderson (2004).

²⁷ Longino articulates this “bottom line” commitment in the context of identifying norms of practice (“community values”) that had informed feminist research in a range of sciences; it is not specifically associated with standpoint theory. None of the norms of practice that Longino described were explicitly feminist but, together, she argued, they function as evaluative standards that “mak[e] gender a relevant axis of investigation”; they serve to “prevent gender from being disappeared” (1994, 481). I develop an analysis of these principles as they figure in feminist social science in Wylie (2012), and I made a case for their relevance to feminist epistemology in Wylie (1995).

²⁸ This discussion of epistemic advantage summarizes an argument originally developed in Wylie (2003), elaborated more recently in discussions of how and why community-based collaborative practice – specifically, archaeological collaborations with Native American communities – can significantly enrich inquiry epistemically (Wylie 2011b).

²⁹ Hartsock later characterizes the formation of a standpoint as a matter of developing an “oppositional consciousness . . . which takes nothing of the dominant culture as self-evidently true” (1997, 96-97).

³⁰ My primary interest was in the epistemic implications of the early critiques of sex/gender bias in archeology (Wylie 1996, 1997b), but this required an understanding of how and why they arose. An account of the formation of “gender archaeology” and an initial analysis of these survey results appeared in Wylie (1992). The conference proceedings were published in 1991 (Walde and Willows).

³¹ This 1989 conference was one of a series of annual “Chacmool” conferences that had been hosted by the University of Calgary since 1966. Each year the student organizers choose a different theme and, prior to the “Archaeology of Gender,” they typically drew 40 to 60 submissions, many of them from regional colleagues who were regular Chacmool attendees. With over 100 submissions, the 1989 conference was substantially larger, and its international reach much greater, than previous Chacmool meetings. Altogether 72% of those listed on the final program responded to my survey, but some of the demographic data reported here (e.g., the representation of women and of international participants) was drawn from the published program. While over half the men presenting at the 1989 conference had attended previous Chacmoos, fewer than half the women reported any previous experience with these meetings.

³² For an analysis of this dissociation of gender archaeology from feminist politics or scholarship, see the introduction to Conkey and Wylie (2007).

³³ In this account (Wylie 2003, 2012) I draw on Longino’s procedural account of objectivity (2002, 128-135), and Lloyd’s extension of her analysis (2005, 241-255).

³⁴ The details of this analysis are available in Wylie (2002, 188-189).